Doubtless it is not necessary, but the obligation is the same; and a mistake into which Prof. Young has fallen is open to others.

The map which he wants—to be based on inverse wave-length or rapidity of vibration or pitch—is, I believe, in course of construction by Dr. Huggins, to whom, and not to me, is due, I think, the first idea and the proposal. Prof. Young has probably associated my name with it through a lecture delivered by my brother at Glasgow, in 1869, in which it was advocated.

I also wish to acknowledge that I was not aware of the fact which has now been so decidedly stated, that the coronal green line is certainly *practically* identical in position with 1474 (K), as tested by direct comparison. Indeed, I was ignorant that such a comparison was possible, having supposed that the line in question was only visible during eclipse. I ought, of course, to have referred to Prof. Young's "Preliminary Catalogue," and probably should have done so had I been in a house instead of in a tent, a few score of miles from the nearest station. But in truth it did not occur to me that there could be any certainty about the position of a line which, as a coronal line, had never been fixed by measurement. I may now venture to ask, What guarantee was there that No. 31 of the "Preliminary Catalogue" was "the coronal line," anterior to the Dodabetta measurement? I do not question it now, but I should like to know if the presumptive identity is supported by any characteristic difference between that line and those which are presumably due to the chromosphere. There is still a link wanting.

However, admitting the identity, and therefore the accuracy of the assigned position, we may still believe what Prof. Young says he would be glad to see proved, that "the apparent coincidence (with the iron line) is merely a very close juxtaposition." More than this: even were a very much higher dispersive power to show no resolution of the identity, should we not still be in nearly the same position as to any inference to be drawn therefrom? Evidence of physical relation between metals which present one or more lines common to both spectra may, indeed, eventually be shown (by the improbability of so frequent an accidental concurrence) to amount to proof. But this must be a prior step. To conceive it taken, and then to apply the like reasoning by analogy to the case of the single coronal line tallying with an iron line, seems to me speculation of the second order. Undoubtedly it would be matter for congratulation to be relieved from the liability to temptation of this kind by definite disproof. In the meantime, I cannot but regret that Prof. Young has half neutralised the good of a plain disavowal of belief in the ferrous interpretation of the coronal green line, by hazarding the query whether it may not "turn out" to be quasi-ferrous.

I am sorry—to return to the subject of nomenclature—that your respondent does not agree to my objection to "D_3." Is it not plain that such a designation is haphazard? The association of idea is with D_1 due to sodium, instead of with the origin or source of the line. It tells nothing beyond the position, roughly, in the spectrum, by reference to a position which we happen to be familiar with, but with the occupant of which it has no connection otherwise. The name, in short, has no foundation in principle; and that, I apprehend, is a lack of the first requisite in a scientific name.

The objection to Greek alphabet letters is of a different character, but not less easily answered. It is very true that, through the exertions of Prof. Young and others, "the whole Greek alphabet would not suffice to name one in three of the lines" already known; but it would nevertheless suffice (as in the somewhat analogous case of the stars) "to express as many as the memory would require to hold." There is ample precedent. The principal lines of the elements, like the principal stars of constellations, are known to some extent by Greek letters; and as for the difficulty in respect of order, there was a time when the "lucid" stars, though very many in number, and having no very clear claims to precedence, were only known individually by personal names. Yet no sooner did a Bayer rank and name them according to apparent brilliancy, by Greek letters (to say nothing of the Roman), than the advantage of a fixed nomenclature was recognised and his work accepted; although observation must have shown that the assigned order was not always strictly correct. So would it happen now if, the lines having been lettered, further knowledge should show that the established precedence was not quite all that could be wished. The evil of slight incorrectness of this kind would be felt to be trifling compared with that which would result from an unsettling of a nomenclature established solely for convenience and involving no theory.

This would not prevent nor conflict with, neither would it render unnecessary, a far more extended tabulation depending on refrangibility. On the contrary, the want of such a classification and means of indication would be felt as soon as precise tabulation should come to be undertaken. In Kirchhoff's solar chart we have, graphically, something like what is wanted in a much more general, numerical, and tabular form—an example of a catalogue of lines. Charts are very useful, but not most handy; and they are not susceptible of such ready improvement and extension. The accumulation of results of spectroscopic research must sooner or later take the form of a catalogue of lines, from all sources, arranged in order of refrangibility; designating individuals (for special reference) where possible, according to their parent element or compound, their physical source, their cosmical habitat, or other characteristic and distinguishing indication, implied under the system of nomenclature which may be adopted; upon which would follow such details as to character (including intensity, width, definition, complexity, variability, &c. &c.) as present knowledge or future research may represent as suitable material for incorporation.

So long as spectroscopic analysis is content to remain in its earliest stages—and it must be allowed to be still in its infancy, though a giant from its birth—the student and experimentalist may to a very considerable extent learn by heart or by practice such spectra as he needs; but this can never suffice for all purposes. Accumulation is continually going on, and products must be stored. Let that be once acknowledged and the task attempted, and it must follow that, no matter how rigorous and precise may be the system of tabulation, there will be not only room, not only gain, but a positive necessity for an intelligible use of that kind of descriptive indication which is only to be found in scientific classification and nomenclature.

A general catalogue such as I contemplate would command, if compiled with even moderate knowledge and care, a very general acceptance. Unquestionably it would be extended, modified, improved upon, by subsequent work; but, so far as nomenclature is concerned, it would probably undergo but slight alteration—the less the better. It would form a basis on which any number of special catalogues might rest, without interfering with its permanence as a catalogue of reference.

I do not pretend to say that the task is a simple one; quite the reverse. But, then, all the more honour to whoever accomplishes it.

J. HERSCHEL

Bangalore, July 29

Jeremiah Horrox

I OBSERVED in a number of NATURE some three weeks ago an inquiry relative to J. Horrox, the astronomer. My wife is descended from Horrox, and I knew that one of her friends had his life, but have not been able to find it till now. The book has just been sent to me here.

The Rev. Jeremiah Horrox was born in Toxteth Park, near Liverpool, in 1619, and died in 1641, aged 22.

The life is by the Rev. A. B. Whatton, published by Wertheim, Macintosh, and Hunt, 24, Paternoster Row, 1859, and includes a translation of his discourse on the Transit of Venus.

HENRY HOLIDAY

Muncaster Castle, Ravenglass, Carnforth, Sept. 20

Millions of Millions

WHY do not Messrs. Ranyard and Co. adopt the late Benjamin Gompertz's most convenient notation of prefixing a circle to the first significant figure, or suffixing a circle to the last significant figure having therein a digit for the number of zeros employed?

Thus: ·⑤718 is ·00000718
And 718⑥ is 718000000

S. M. DRACH

74, Offord Road, N., Sept. 17

Analogy of Colour and Music

IN NATURE, No. 150, p. 393, a letter from Mr. G. C. Thompson is headed "Correlation of Colour and Music." As this letter refers to a paper of mine published some time ago, permit me just to say that Mr. Justice Grove has in your journal objected to the use of the word "correlation" employed in this sense. Entirely coinciding with the opinion of the eminent parent of this

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work, I wrote as follows in NATURE for February 17, 1870:—"*Analogy* is certainly far more appropriate to express what is merely a parallelism, and not a necessary or complementary relationship between light and sound." In the subsequent letter on this subject you adopted the word "analogy;" pardon, therefore, my pointing out an obviously accidental "reversion to the primitive type" which appeared in your paper Sept. 12.

Sept. 16 W. F. BARRETT

The Fringes on the Lighter Side of the Rainbow

AT the place referred to by Mr. Thompson in NATURE (No. 150, p. 393) I merely followed Sir John Herschel; expressing myself, it is true, not very accurately, in my anxiety to save space in NATURE at the end of a letter already too long. If Mr. Thompson refers to Sir John's "Meteorology," sections 219 to 224, and still thinks the point requires fuller elucidation, he may possibly supply the deficiency by devising an experiment to prove that the width of the fringes does not vary inversely as the diameter of the drops.

In answer to Mr. Thompson's concluding question, perhaps somebody else will furnish the latest intelligence. I do not know what has happened in the last five years, and I do not know what you call violet; but I believe that in 1867 the extreme rate of vibration for visible rays was about 801 million millions a second. C. J. MONRO

A Curious Phenomenon

A VERY curious phenomenon was witnessed here on Wednesday afternoon last, September 4, about three o'clock, in a westerly direction. A somewhat heavy thunderstorm, originating towards the south, had divided its fury before reaching this immediate neighbourhood, one branch passing N.E. towards the Pennine Hills, the other taking the N.W. course, that to the N.E., however, being more violent. As the storm was passing, a stream—*apparently* of water, and fully six inches in breadth—shot with considerable speed from the vicinity of a dark, fiery cumulus across a rain cloud of a very deep blue, murky tinge. Its passage, as witnessed by my boy from its commencement, was similar to that of a rocket, at firs assuming a quivering motion, then darting suddenly forward, for some distance horizontally, afterwards obliquely. Its apparent length would be fully twenty yards, being of a very light slate colour. After I saw it the phenomenon remained about two minutes; but its total duration would be not less than five, vanishing gradually during its whole length.

Whatever the phenomenon itself—or its cause, its upward course was certainly very striking, and to me unprecedented—the impression on some people's minds being that it was water drawn up from Lake Ullswater into the clouds by the lightning!! A terrific storm of thunder and lightning occurred on the previous evening at 9 P.M., when several fatal accidents were reported. T. F.

Blencowe School, Cumberland, Sept. 7

APPEAL FOR SKELETONS OF WILD SPECIMENS OF THE LARGER CARNIVORA FOR OUR MUSEUMS

NEITHER in the Museum of the Royal College of Surgeons nor in that of the University of Oxford is there a skeleton of a wild lion or a wild tiger, and it is probable that there is no such skeleton existing in England. The preparations in our Museums, illustrating the anatomy of the larger carnivora, are almost without exception derived from menagerie specimens.

Lions breed well in confinement, and hence an ordinary menagerie specimen may not only itself have been during its whole life confined in a cage, but its ancestors may have suffered a like fate. At all events it has been trapped whilst still young, and reared in confinement, as is usually the case with the menagerie tiger. Now an animal confined in a narrow space from its youth upwards never has free play for its muscles, and as its food is provided for it, is never called upon to exert them in a violent manner. The result is that the bony framework on which the muscles act never attains in such specimens its full development, and the ridges and inequalities on the bones corresponding to the origin and insertion of the muscles are not well marked. Moreover, menagerie animals, as is well known, very frequently suffer from diseases of the bones, and the marks of these diseases may be seen on many of the skeletons in every anatomical museum. Now, it is of great importance to possess perfect skeletons of adult wild large carnivora, both for general study, and more especially for comparison with the remains of similar carnivora which are to be found in the more modern geological deposits in Great Britain. Considering the number of tigers and lions which are annually killed by English sportsmen, it is surprising that this desideratum has not yet been supplied. The reason probably is that sportsmen generally do not know that it exists, or do not understand how a skeleton should be prepared. The sportsman is usually content with preserving the skin of his tiger or lion; but no doubt there are many who would gladly aid the cause of science by preserving the skeleton as well, if they knew how much the result of their labours would be valued at home. I propose here to give a few simple directions for the rough preparation of skeletons for transmission to England, merely premising that I trust that if any sportsman may be induced by reading these notes to send home a skeleton, that he will send it to the Oxford Museum, in which I am especially interested, and I hope some old University man may help us in this matter. Any packages should be addressed to Prof. Rolleston, Museum, Oxford. Skeletons of other wild animals are, of course, of great value, and will be most gladly received; they also are too frequently only to be got from menageries.

Directions for Preparing Skeletons.—The skin having been removed from the animal, the abdomen should be slit open, and all the viscera extracted. The limbs should then be severed from the body, the scapula or blade bone being left attached to the fore limb, the hind limb being removed at the thigh joints, and care being taken that the articular surfaces are not injured in the process. The flesh should now be removed roughly from each of the limbs with knives; the several bones which go to form each limb should if possible be allowed to remain attached to one another. On no account should the small bones of the hind or fore foot be separated from their attachments. Mr. Flower, indeed, advises that the skin be not removed from the feet at all. The limbs being thus roughly cleaned, they should be placed in water for several hours to allow the blood to soak out, and they should then be placed in the sun till dry. The head should be disjointed from the neck, and the flesh cut off it. It is most convenient to commence with the strong muscles of the jaw. After these have been cut through, the ligaments which hold the lower jaw in place may be divided, and it may be separated from the skull. The tongue may now be removed, and search must be made in its base for several small bones constituting the hyoid apparatus, which should be carefully taken out, and tied at once to the lower jaw for fear of loss. A considerable quantity of the brain may be removed by means of a spoon-shaped stick through the aperture at the back of the skull where it joins the neck. The rest may be removed by means of large shot put in at the hole, and shaken up with water. The neck may be cut off close to the trunk, and the tail close to the rump, and the flesh removed with the knife. The chest cavity should be left entire, the flesh being removed as well as circumstances will permit. The whole of the pieces should be treated with water, and then dried, as in the case of the limbs. The skull, limbs, tail, and neck may be conveniently placed inside the chest cavity for packing, and if it be necessary to get the skeleton into a short packing case the back bone may be divided behind the chest cavity, and the hinder vertebræ and hip bones laid along side of

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